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Proceedings at Boston, May 20th, 1874.

THE Society held its Annual meeting, as usual, in the Library of the American Academy, Athenæum building, Boston, at 10 o'clock, A. M., the President in the chair.

The Treasurer's report for the year was read, and, having been referred to an auditing Committee, was examined and accepted. It shows the income and outgoes of the year to have been as is stated below :

RECEIPTS.							
Balance on hand, May 21st, 1873,	-	-	-	-	-	-	\$906.09
Annual assessments paid in,	-	-	-	-	-	\$565.00	
Sale of the Journal,	-	-	-	-	-	84.84	
Interest on deposit in Savings Bank,	-	-	-	-	-	62.82	
							712.66
Total receipts of the year,	-	-	-	-	-	\$1,618.75	
EXPENDITURES.							
Printing of Proceedings,	-	-	-	-	-	\$53.36	
Expenses of Library and Correspondence,	-	-	-	-	-	35.39	
Total expenditures of the year,	-	-	-	-	-	88.75	
Balance on hand May 20th, 1874,	-	-	-	-	-	1,530.00	
							\$1,618.75

A general report as to the increase of the Library during the year was presented, the details of accessions being left to be given in the printed Proceedings.

The Directors announced that, in consideration of the non-appearance during the year of any continuation of the Journal, they had voted that no annual assessment be levied on the members for the year 1874-75. Further, that they had appointed the Semi-annual meeting to be held in New York, on Wednesday, October 28th, designating Prof. Short and Dr. Ward of New York, with the Secretaries, a Committee of Arrangements for it.

The following gentlemen were then nominated by the Directors as candidates for Corporate membership, and were duly elected by ballot :

Prof. Felix Adler, of Ithaca, N. Y. ;
 Mr. Isaac H. Hall, of New York ;
 Rev. Henry F. Jenks, of Boston ;
 Rev. Howard Osgood, of New York ;
 Prof. Charles P. Otis, of Boston.

The election of officers for the ensuing year being now in order, Messrs. Trumbull of Hartford, Merrill of Andover, and Ward of New York were appointed a Committee of nomination. They reported the names of the following gentlemen, who were balloted for and duly elected :

<i>President</i>	—Prof. E. E. SALISBURY, LL.D.,	of New Haven.
<i>Vice-Presidents</i>	{ Rev. N. G. CLARK, D.D.,	" Boston.
	{ Hon. PETER PARKER, M.D.,	" Washington.
	{ Rev. T. D. WOOLSEY, LL.D.,	" New Haven.
<i>Recording Secretary</i>	—Prof. EZRA ABBOT, LL.D.,	" Cambridge.
<i>Corresp. Secretary</i>	—Prof. W. D. WHITNEY, Ph.D.,	" New Haven.
<i>Secr. of Class. Sect.</i>	—Prof. W. W. GOODWIN, Ph.D.,	" Cambridge.
<i>Treas'r and Libr'n</i>	—Mr. ADDISON VAN NAME,	" New Haven.
<i>Directors</i>	{ Mr. J. W. BARROW,	" New York.
	{ Mr. A. I. COTHEAL,	" New York.
	{ Prof. W. H. GREEN, D.D.,	" Princeton.
	{ Prof. A. P. PEABODY, D.D.,	" Cambridge.
	{ Dr. CHARLES PICKERING,	" Boston.
	{ Prof. CHARLES SHORT, LL.D.,	" New York.
	{ Rev. W. H. WARD, D.D.,	" New York.

The Corresponding Secretary called the attention of the Society to its losses by death during the past year. The list included the names of three Corporate members,

Mr. Charles Astor Bristed, of New York,
Prof. Alpheus Crosby, of Salem, Mass., and
Col. James F. Meline, of Brooklyn, N. Y.;

and of one Corresponding member,

Dr. Francis Mason, missionary in Burmah.

The Secretary gave a somewhat full sketch of the life and literary labors of Mr. Bristed, who had for some years been a member of the Society, though never taking a personal part in its proceedings; he also spoke, more briefly, of Col. Meline, elected only a year since to its membership. Dr. Peabody bore testimony to the high scholarship of Prof. Crosby, and to his eminent services in the cause of education. Dr. Anderson and Dr. Ward eulogized the character of Dr. Mason, and described his work, both as missionary and as scholar. He had, in some of his visits to America, been present and offered communications at the Society's meetings, and had repeatedly, and down to a very recent period, furnished articles to its Journal. His principal scholarly works are a volume of various information on Burmah, and an edition of Kaccâyana's Pali grammar, which forms a part of the series of the Bibliotheca Indica, published at Calcutta. He was born in 1799.

The correspondence of the half-year was presented, and some extracts from it were read. Dr. C. H. Brigham, of Ann Arbor, Mich., had sent an account of the finding of a little manuscript roll, in Ethiopic, on the premises of the Michigan Central Railway at Jackson Junction, apparently dropped there by some traveller. He added a full description of the MS., which is evidently not very old, is well preserved and neatly written, and illustrated with several pictures. It is a liturgy.

Rev. T. C. Trowbridge, missionary in Asiatic Turkey, being present, addressed the Society on the college of Western learning

now sought to be established at Aintab; and also spoke of the many and rich opportunities in that region for antiquarian research.

Communications were now called for and presented.

1. On a Greek Inscription from near Beirût, published in the Second Statement of the American Palestine Exploration Society, October, 1873, by Prof. F. P. Brewer, of Columbia, S. C.

The following is proposed as a revision of the last six lines. The preceding part of the inscription is very faulty, but seems to give the name of a Phœnician who obtained the sovereignty of Heliapolis.

- | | |
|----|---|
| 5 | Αἰψα μάλ' ἐκτελέων [ἐργ'] ὅσσα νόφ φρόνεε |
| 6 | Φοινικῇ αὐτῇ, ὅσον καὶ τάδε ἐργ[ον], |
| 7 | [Ἀσ]τεο[ς ἐν]ρο[ί]η [λ]αῶ μέγα θαῖμα τ[έ]λεσσευ |
| 8 | [Ἰ]ς [κ]ατὰ τῶν σκοπέλων ἴσον ἐθηκε μέσον |
| 9 | Ὅφρα διηνεκῶς ὁμαλὴν ὁδὸν ἐπανώντες |
| 10 | Φείγωμεν χαλεπ[ῆ]ς ὕψος ὁδο[ι]πλαν[ί]ης. |

Translation. 'While very promptly executing whatever works he devised for Phœnicia herself, so great a work even as the following did he finish from good will to the city, a great wonder to the people, when down from the peaks midway he made an even path, in order that by using a road that was level throughout we may avoid the height of painful wandering.'

The letters in brackets are the only ones that vary from the published text, which in most of those places was "recognized with difficulty." We have changed ZO to AΣ (7), and EI to H (10), and have inserted EΥ (7), K (8), and the *iotas* in lines 7 and 10.

The measure is the elegiac distich. The only irregularity in the last four lines would be removed if we could read ἐπανώντες at the end of line 9.

The road which our inscription commemorates seems to be the one referred to in a well known Latin inscription in the vicinity, which says: *M. Aurelius Antoninus montibus imminentibus Lico flumini caesis viam delatavit.*

2. On the use of 𐤁 in Hebrew with Negative Particles, by Prof. C. M. Mead, of Andover, Mass.

The object of this investigation was to ascertain, if possible, by what means the Hebrew language distinguished a partial from a universal negation. To this end an attempt was made to make a complete list of all the passages in which 𐤁 is used with negative particles, and to classify them according to the position of 𐤁 in the sentence, and according to its being definite or indefinite. Noldius, in his *Concordantiae Particularum*, adduces only ninety-three passages, dividing them into two classes: those in which 𐤁 precedes, and those in which 𐤁 follows, the negative. He finds three passages, Num. xxiii. 13, Deut. xviii. 1, and I Sam. xiv. 24, in which the negation is designated as partial. But the two latter are as clear instances of universal negation as any that could be found. Num. xxiii. 13 is the only passage referred to by Ewald and Gesenius as exhibiting a partial negation, though they make the impression that there is something like a consistent principle governing the matter—Gesenius affirming (or implying) that, when 𐤁 is made definite, the negation is partial; Ewald, that the negation is partial when 𐤁 is equivalent to *totus*, as distinguished from *omnis*.

The result of the investigation is that both of these representations are inaccurate, and that there is no law of construction determining the question. Of the 326 cases examined, only six present unequivocal instances of partial negation. Of these six, it is true that five occur in sentences in which 𐤁 is definite; but the vast majority of instances in which it is definite exhibit universal negations; and in Lev. xvi. 2, where it is indefinite, the negation is clearly partial. The other five sentences are Num. xxiii. 13, Josh. vii. 3 (*bis*), I Kings xi. 13, 39. Of these six cases, three are found with 𐤁, and three with 𐤁, connected with 𐤁.

There is, however, a class of sentences in which the negation cannot be regarded as strictly partial or strictly universal, but rather as a negation of a universal affirmation. E. g. Gen. viii. 21, "I will not again smite any more every thing living." Here it is declared that there will not be another universal deluge; whether some or none will hereafter be destroyed, is left undetermined. Of such passages about twenty-five may be found, though it is manifest that the line of distinction between such negations and either of the two other classes must be somewhat indeterminate. A few of them border upon the partial negation: viz. Num. xi. 14, Eccl. vii. 21, Is. lxxv. 8, I Chron. xxix. 34.

It might have been anticipated that, as in English, a partial negation would be most unambiguously expressed by prefixing the negative particle immediately to the word denoting universality, instead of having the verb intervene between them. But, singularly enough, there are no instances of this position of the words in all the Hebrew Scriptures. The two apparent exceptions (I Kings xi. 39 and Ps. cxv. 17) are only apparent; for, in both, the construction is elliptical, and a verb is to be supplied. It is true, however, that in the analogous construction of לא with עולם (usually לעולם), whenever (as happens in four passages) the negative immediately precedes לעולם, the combination has the meaning 'not always;' whereas, out of the thirty cases in which these words are separated by a verb, in twenty-seven the combination unmistakably means 'never;' and in only one of them (Lam. iii. 31) does it express a partial negation unequivocally. One can hardly resist the conclusion that in the spoken language the same distinction may have existed in regard to כל; but, as the matter now stands, we can only say that, so far as the extant literature is concerned, the general law is that כל with negatives expresses a universal negation; the exceptions are ascertained only by the sense of the passage or of the context.

3. On the Chinese *sieu* as Constellations, by Prof. W. D. Whitney, of New Haven.

Prof. Whitney spoke on this subject somewhat as follows:

By an oversight of the learned editor of the new edition of Colebrooke's *Essays* (Prof. Cowell, of Cambridge), I find myself there quoted (vol. ii., pp. 281, 282) as favoring Biot's opinions respecting the history of the Chinese system of *sieu*, and the derivation from it of the corresponding system of Hindu *nakshatras*. The quotation is from the notes to the *Sūrya-Siddhānta*, published in the *Society's Journal*, vol. vi., 1860. Four years later than that, however, in a special article on the subject, printed in the eighth volume of the *Journal* (first part, 1864), I explicitly and entirely rejected Biot's view, and did my best to prove its untenability from the data which he himself furnished—as it seemed to me, with satisfactory success. I am led to revert once more to the subject, partly in order to reiterate my confidence in my later argument and its result; but chiefly in order to call attention to certain sources of information, not then accessible to me, which leave no further doubt or question respecting the matter.

Biot everywhere defines and describes the *sieu* as single determinative stars, selected by the ancient Chinese as standards of reference for observations on other stars, because, being situated near the equator of B. C. 2350, they nearly coincided in right ascension with the principal circumpolar stars, which the still earlier Chinese had been in the habit of observing with particular attention. So far as I know, he lets slip only at a single point so much as a hint that any one had ever thought of the *sieu* as constellations. At the foot, namely, of his second table, in the series of articles in the *Journal des Savants* of 1840 (and repeated in that of 1861), he gives the meaning of some of the *sieu*-names, nearly all of which would fit groups better than single stars, while of one he says: "the Chinese character for Pi means 'the snare' (*le filet*), which is the figurative designation of the Hyades." I drew attention to this as a pregnant indication in my later article (*Journal*, viii. 43), and remarked that, in view of the Indian and Arabian aspects of the system, it might be dangerous to assume that, when an early Chinese authority names a *sieu*, only the single star can be meant which the later astronomers know by that name; or even that the division of the heavens, where one is implied, is to be reckoned from star to star. And not, as in the other two systems, by simple proximity to the asterism named. And authorities which I am now able

to cite raise this suspicion to a certainty. Thus, in the first place, Gaubil, the founder of European knowledge of Chinese astronomy, always speaks of the *sieu* as "constellations," and here and there defines the groups of which one or another is composed. So, for example, in Souciet's collection, vol. iii., p. 32 : "One sees still that the constellation Fang [fifteenth *sieu*, β , δ , π , ρ Scorpionis] is so well pointed out by the number of four stars of which it is composed, and of which the bright one (*la Lucide*) is the chief." Again, M. Am.-Sédillot, the eminent Orientalist and mathematician, in his *Mémoires pour servir à l'Histoire Comparée des Sciences Mathématiques chez les Grecs et les Orientaux* (second part, Paris, 1849), gives the whole series of groups, and repeatedly points out that, "when the determining stars, which have suggested so many considerations, so many calculations, so many lofty hypotheses, are restored to the constellations of which they form a part, and which the Chinese themselves have adopted, we see reappear as if by enchantment the various parts of the Arab system, and are obliged at once to acknowledge that we have here really the twenty-eight lunar stations, and by no means divisions that are independent of the movements of our satellite." And once more, in a quite recent and independent work, by Mr. John Williams of London, entitled *Observations of Comets, Extracted from the Chinese Annals* (4to, London, 1871), the author, in the course of his Introductory Remarks on the Chinese astronomy in general, expresses himself as follows (p. xxi.): "the Chinese divide the visible heaven into thirty-one portions; twenty-eight of these may be termed the stellar divisions, and receive their names from, or are determined by, an asterism, generally forming the central or principal one of the division. The determination by an asterism having the same name has been preferred by me to that by any particular star in that asterism, as being, to the best of my judgment, more in accordance with the Chinese mode of proceeding; in which, as far as my experience goes, the asterism alone is mentioned, and not any particular star in that asterism." And to the same effect later (p. xxvi.). Mr. Williams's definition of the asterismal groups accords quite closely with that of M. Sédillot. He does, indeed, report also the series of determining stars; but he gives them as "according to Biot"—apparently, as finding no more ancient or genuinely Chinese authority on which to rely for them. And in the appendix to the work he presents a series of little star-charts, taken from native sources, in which each asterism is set down, in company with the other groups belonging to that division of the heavens to which the asterism gives name—the division being, as in the Hindu system, the circumjacent region, though not an equal twenty-eighth part of the ecliptic.

In these statements, now, is evidently implied the complete and irretrievable overthrow of Biot's view as to the *sieu* and their history; it has not a single leg left to stand upon, if the *sieu* are constellations and not determinants. And I find it extremely hard to understand how a *savant* who has shown elsewhere such simple and entire good faith in his own expositions and reasonings, often himself putting into our hands the means of refuting his errors, should have allowed himself at this point to ignore and omit a very important part of the evidence bearing upon his case. That he did not believe himself to be acting in good faith here also, I have not the least disposition to suggest; but great indeed must have been his prepossession, to warp his judgment to such an extent. The whole subject was one upon which he had an intense personal feeling, conceiving that his statements and arguments had been treated with undue disregard and disrespect by the Indianists, and that he had no justice to expect at their hands; and he was so under the dominion of preconceived opinion as to be incapable of receiving new light. His view of the Hindu system of *nakshatras* was wholly and perversely wrong, and even in his articles upon the *Sūrya-Siddhānta* he passed without the least notice alike the general (provisional) assent to his theory which it contains, and its specific objections to certain points in that theory. It must, I think, be conceded that, whatever may be in other respects his deserts as to the history of Chinese astronomy—of that I am no competent judge—his discussion of this particular institution has absolutely no value; so far as it is concerned, he has justified the worst of the suspicions expressed by Weber, which he resented so highly; he has added one more to the long list of those able mathematicians who have shown a disabling incapacity to discuss questions involving historical and documentary as well as scientific evidence.

It is greatly to be wished that some competent Chinese scholar would take up the *sieu* as Weber took up the *nakshatras* in his essays published in the Transactions of the Berlin Academy for 1860 and 1861, making a like thorough exhibition and discussion of their character and aspect as exhibited in the Chinese literature. Although we have no right to hope that it would cast valuable light on the ultimate origin of the institution, it would at least lay a solid foundation, such as is now painfully wanting, for the study of this important element in the ancient Chinese science.

In connection with this subject, it may be well to call attention for a moment to the untrustworthy manner in which nearly all questions relating to the ancient Hindu astronomy and its connection with chronology are treated by Lassen, even in the second edition of his great work, the *Indische Alterthumskunde*. He is apparently too unfamiliar with astronomical discussions to be able to use the various materials which have been published on the subject, distinguishing the false from the true, the unsound deductions from the sound. But he is also unreasonably careless; as this example, among others, will show. "It is," he says (i. 983), "a remarkable circumstance, that the description of the equator in the *Sūrya-Siddhānta* corresponds to the year 2350 or 2357 B. C. How this phenomenon is to be explained, I must leave to the astronomers." And he refers, as authority for the astonishing statement, to the Society's Journal, vi. 467 and viii. 157 (where doubtless, for 157, we are to read 37). But as the *Siddhānta* does not describe any equator, and as there is nothing on the two pages quoted, or anywhere else in the Journal, in the most distant manner intimating that it does, or connecting anything in the treatise with the date specified, the astronomers will do well to decline the task thus put upon them.

Again, I objected, some years ago (Journal, viii. 68, note), to Lassen's assertion in his first edition that Colebrooke had "shown" (*dargehen*) the Arabs to have received their lunar zodiac from the Hindus; now, in the second (i. 979), he declares Colebrooke to have "proved" (*nachgewiesen*) the same thing. The fact is simply that Colebrooke, after really "showing" or "proving" the Hindu and Arab zodiacs to be varying forms of one system (a fact which had been doubted or denied before), declared that he thought it more probable that the Arab system came from the Hindu than the contrary; and, though he later repeated the same opinion in a more confident tone, he never entered into any argument upon the matter. He was not in a position successfully to discuss and solve the question; and his mere expression of opinion, in virtue of the great additional light since cast on it, especially by the bringing in of the Chinese *sieu* as third term in the comparison, has no authority whatever.

Once more, Lassen pronounces (i. 607, note) the accuracy of Pratt's determination of the date of the *Jyotisha* as 1181 B. C. to be beyond doubt or question. This is perhaps less to be wondered at. Considering the rarity and preciousness of a definite date in ancient Hindu history, we must not expect to put down this one, with however good argument. For two or three generations longer, at least, it will continue to be claimed, either that the date of the *Jyotisha*, by scientific demonstration, is precisely 1181 B. C., as determined by Jones and Pratt, or that it is precisely 1391 B. C., as determined by Davis and Colebrooke. It will not be possible to make people see that both these dates are just equally valuable—or worthless. Yet the argument lies in a nutshell. In about the sixth century after Christ, having learned scientific astronomy from the Greeks, the Hindus made observations on the positions of 28 stars, as measured from the vernal equinox of that period. These observations are so coarse and inexact as to show an extreme discordance of $5\frac{1}{2}^\circ$ from one another, when tested by our modern methods; and each one of them, used as a starting-point for chronological calculations, will give a different result, the extreme results being about four centuries apart. Davis and Colebrooke took one star of the twenty-eight, and it brought them, they thought, to 1391 B. C.; Pratt took another, and it brought him to 1181 B. C.; still others might have been taken which would have given as result 940 B. C.; and the rest would fall in here and there between these extreme dates. And behind this uncertainty of four centuries there remains still the fact that the attempted measurements are from the equinox of the 6th century of our era, which we have no reason for regarding as having determined the asterismal division of fifteen or twenty centuries earlier—even if there had been any precise system of division



Fig. 5.



Fig. 1.



Fig. 4.



Fig. 2.



Fig. 6.



Fig. 3.

then, which there was not, and could not have been. The Jyotisha is in reality utterly worthless as determining any date in Hindu history, and the sooner Indian scholars come to recognize the fact, and cease to lean on such a broken reed, the better will it be for their study.

4. On the Phœnician Inscriptions in the Cyprus Collection of Di Cesnola, by Rev. W. H. Ward, of New York.

Among the objects collected by Consul Di Cesnola in Cyprus, and now deposited in the Metropolitan Museum in New York, are a number of fragments of marble engraved with Phœnician letters, and a very few earthenware jars with brief Phœnician inscriptions. One of the latter, and most of the former, have been published in fac-simile by Dr. Paul Schröder, author of the Grammar of the Phœnician Language. They had previously been examined by Prof. Rödiger, but his copies do not seem to be exact, and I have not seen them. My object is simply to add a few brief inscriptions not already given by Schröder.

As described by him, the two larger inscriptions are on cubes of marble. Others are on the flat upper rim of marble basins, a foot or two in diameter, which were placed as votive offerings in a temple at Citium. The inscription contained the date, being the regnal year of the king Melchiyathon, or Pumiyaathon, his son who reigned in the fourth century B. C., and the latter of whom is identified with the Pymathos of history. The inscriptions are generally but fragments, of a very few letters, and add almost nothing to history. The god Resheph is confirmed.

Schröder's "No. 15" should receive the addition of a fragment (Fig. 1) containing the two letters conjectured by Schröder, namely, כר, giving the whole כר[מל] קרת 'May Melkarth bless.'

On a fragment of the polished rim of a gypsum bowl is found the following inscription (Fig. 2), not given by Schröder. [מ]לך כתי ואד[ל] 'King of Citium and Idalium.' It is remarkable for nothing but for the delicacy of the inscription, which is simply scratched with a point on the polished gypsum, and is as perfect as when first made. It is an extremely fine specimen for giving the exact shape of the letters in their thinnest outline. We notice here very distinctly the peculiar shape of כ, found also on the long inscription (Di C. 1), given wrong by Schröder in his plate, and differing sharply from any form given by Schröder in his Grammar. It is made with the right hand perpendicular line completely disconnected from the other two. The fact that the same form is found in Di C. 1 is evidence that this also belongs to the reign of Pumiyaathon. In the inscription of Melchiyathon we have the ordinary shape.

On the rim of another bowl we find the following (Fig. 3):

[ל]אדני לאשמן מל[קרת] 'To my lord Eshmun Melcarth.'

Besides these, there are new inscriptions on three earthen jars or vases. The inscription on all of these is written carelessly and baked in the clay. That given in fig. 4 is בעל, 'my master,' or possibly a mere form of 'Baal.' It is on a jar over two feet high, ending in a point below. Fig. 5 represents a jar of similar size and shape, with an inscription in three lines, illegible in the sixth letter of the first line, and with the fourth letter of the same line doubtful. It reads as follows:

[שמע] | ית | בעל* | Baal-, presented by Shimei.'

The name of the god is not determined, and appears to differ from any of the familiar forms of Baal.

The inscription on the very beautiful vase given in Fig. 6 has its first letter partly covered by the lower circle of the ornamentation. I am utterly unable to give a satisfactory or even a plausible reading of it.

5. On the Pseudo-Phœnician Inscription of Brazil, by Rev. W. H. Ward.

Dr. Ward added to his preceding paper a few remarks on the so-called Phœnician inscription of Brazil, speaking nearly as follows:

A year or two ago, the Brazilian papers reported the discovery at a place called Parahyba of a Phœnician inscription. It has since been published in fac-simile

in the number for April 23, 1874, of the Portuguese illustrated paper, *O Novo Mundo*, published in New York; the fac-simile is accompanied by a Portuguese translation. It is so inherently improbable that a Phœnician inscription should appear in Brazil, that we are justified in receiving this one with great skepticism. An examination of this inscription does not relieve it of the suspicion of forgery. The language is not that of other Phœnician inscriptions, but is a mixture of Hebrew and Chaldee. The appearance of **עליונם ועליות** is alone enough to condemn it as a forgery, this form being evidently copied from Gesenius, as a restoration from Plautus. The true Phœnician would be **ערנם ועלנת**. Such forms as **נהיה** and **ננסע** are inadmissible, and the appearance of the word for 'ten' in two forms, **עשר** and **עסר**, is very suggestive of a careless Jewish forger. The occasion of this forgery may be conjectured to be the bitter contest going on for some time between the clergy and the freemasons of Brazil, whom the priests have excommunicated, and to whom they have denied Christian burial. It is not unlikely that some unscrupulous person should have concocted this inscription, recording that King Hiram's subjects entered Brazil, for the purpose of connecting the land with the reputed founder of freemasonry.

At this point the Society took a recess, reassembling at 2 o'clock p. m., when the reading of communications was resumed.

6. On Recent Discussions as to the Phonetic Character of the Sanskrit *Anusvāra*, by Prof. W. D. Whitney.

The nasal utterance called the *anusvāra*, Prof. Whitney said, is an element in the Sanskrit system of articulate sounds as to the value of which there has prevailed some doubt and difference of opinion. Was it a nasal tone accompanying the utterance of a vowel, a nasalization of the vowel, as in the ordinary French pronunciation of *en, on, un*? or was it a distinct nasal utterance following the vowel? This difference of opinion began with the Hindu grammarians themselves. Of the four Prātiçākhyas, one, that to the Atharvan, takes the former view, acknowledging only nasalized vowels in its alphabet; another, the Tāittiriya-Prātiçākhyā, is uncertain and inconsistent; it acknowledges an *anusvāra* as independent alphabetic element, but when it should come to prescribe it as occurring in certain situations, it prescribes the nasalized vowel instead, and merely adds that some teach a nasal utterance *after* the vowel instead of *in and with* it. The other two acknowledge both nasalized vowel and separate nasal, but teach the latter in the great majority of cases: the Vājasaneyi-Prātiçākhyā, again, mentioning authorities that hold the other view. The predominance of authority, it is seen, is on the side of the *anusvāra* as appendage instead of accompaniment to the vowel. And this view is adopted by Pāṇini, and so becomes the orthodox doctrine of later Hindu grammatical science.

Of course, now, this difference of opinion may be the result of an actual difference of pronunciation of the element in question in various parts of India, or schools of Vedic study. But, as I hold, it is equally obvious and undeniable that it might be the result of a different apprehension and theoretical explanation of the same utterance—such as is not infrequently met with among the Hindu phonetists on other points also; not to speak of the differences even among the best modern European scholars, as regards, for instance, the distinction between surd and sonant consonants, or the question whether the nasals *n* and *m* are explosives or not. Nor are we driven to accept as conclusive on the subject the final unanimity of the later Hindu grammarians; in the ever-increasing artificiality of the scholastic pronunciation of a dead language, it is by no means impossible that a false theory should finally prevail, and should come to govern the later utterance: there are striking illustrations of this, as I think, in other parts of the Hindu system.

In a note on a passage of the Tāittiriya-Prātiçākhyā (ii. 30: p. 66 ff.), I set forth the discordance of the authorities, pointed out the alternative ways in which it could be explained, and, without assuming to decide the case, indicated my provisional inclination to regard the discordance as due to a difference of apprehension rather than of utterance, and, as between the two views, to side with that of the

Atharva-Prātiçākhyā. And this, mainly for the reason that the Hindus give no intelligible and acceptable explanation of the *anusvāra* as a separate element, and that it appeared to lack analogies in the usage of other languages; in which, on the other hand, nasalized vowels as consequences of the deconsonantizing (so to speak) of a nasal mute are not infrequent. I expressed no absolute opinion, and left the matter fully open to be reargued by any one else who could show the probabilities to lie otherwise than as I viewed them.

A French scholar, M. Bergaigne, takes up the subject in the *Mémoires de la Société de Linguistique* (ii. 31 ff.), and looks at it in a very different manner. The suggestion that the wide disagreement of the oldest Hindu authorities entitles us to sit in judgment over them, inquiring whether or not they were taking discordant views of the same thing, before proceeding to decide which view was correct, or whether both were so, he rejects; or rather, he utterly ignores it; it does not enter into his argument at all. He speaks of the Atharva-Prātiçākhyā as "confounding" the true *anusvāra* with the nasal vowel; he rules the Tāittiriya-Prātiçākhyā out because of its "inconsistency"—as if this inconsistency were not an important item in the case; he curtly denounces my implication that those who taught the separate *anusvāra* may perhaps have really uttered a nasal vowel, as an unauthorized extension of the usage of the Atharva-Prātiçākhyā to the other treatises. My exposition of the three possible alternatives, and guarded expression of preference for one of them, he describes as catching a glimpse of the true solution, but strangely preferring the violent measure of blotting out the *anusvāra* from the list of Sanskrit sounds!

When a man is so dogmatically confident as this, and (as I think I may claim) so unfair to his predecessors, he may properly be expected to make out, for his justification, a very strong case in favor of his own view. But I cannot see that M. Bergaigne does this at all. When he proposes to "demonstrate" that the *anusvāra* is nothing but a nasal resonance following the vowel, he also proposes to do it by quotations from the Hindu grammarians; and he proceeds to cite a whole series of passages from the Rik-Prātiçākhyā; most uselessly, as every one knew before what theory that treatise takes and (nearly) consistently upholds. He hints, indeed, at "a hundred" others; but we see nothing of them in his pages. He also has his physical explanation to offer; but it is so blindly and inaccurately stated as to be unusable. Thus, he speaks of the nasal resonance as accompanying the pronunciation of a nasal consonant, "in the same manner as the pronunciation of the other sonant consonants is accompanied by a resonance of the glottis;" as if a glottal resonance were not necessary also to the utterance of every nasal. He says the nasal resonance is not necessarily bound to the nasal consonants and vowels, but may be heard by itself, as in humming, or in the *n* of English *mutton*; while in fact these two are different and distinctly consonantal sounds. Finally, he refers to the usage of certain provincial districts of France, where the syllables which in ordinary French have nasalized vowels are claimed to have instead a nasal addition to the vowel. This is really interesting and important, and the fact may not be left out of account in any future discussion of the subject. But it is, to my apprehension, the only valuable item which M. Bergaigne brings to the discussion, and it is very far from warranting the superior tone in which he makes his statements and decisions.

In a later note (p. 199 ff.), M. Bergaigne lets us see more clearly what his physical theory really is. A closure of the mouth-organs in the *k* or *g* position, with nasal resonance, produces the *ng*-sound, down to the lowest point where *k* or *g* can be made. But, he holds, a nasal may be made still farther back and down, because the veil of the palate, when dropped to unclose the nasal passages, can reach and make contact with the base of the tongue at a point yet lower. And a nasal resonance thus produced, having the opening of the mouth cavity, and the auxiliary buccal resonance, reduced to their very lowest terms, would be the pure nasal resonance with which we are to identify the *anusvāra*. The obvious objection here is that the sound thus produced, as implying a closure of the mouth-organs, would still be a nasal mute—and so, as M. Bergaigne claims, essentially an explosive sound—though one to which there was no corresponding ordinary sonant and surd. And any closure of the organs is, to my mind, inadmissible in an account of the *anusvāra*. The essential character of this sound consists in its assimilation to the consonant that follows it. For example, a final *m* before a mute of any class becomes the

nasal of that class; before a semi-vowel (except *r*), it becomes, the Hindus say, the corresponding nasal semi-vowel: that is to say, an emission with nasal resonance in the same position of the organs in which sonant emission without such resonance makes the semi-vowel; there is no difference of articulating position between the *anusāra*-like element and its successor. And I, for one, am not yet ready to believe that, before *r* and the spirants, there is a definite position of closure taken up; I hold it much more likely that in saying *aīṇa*, *aīṣa*, etc., for example, the organs shut only from the open *a* to the comparatively close sibilant; and that the nasal element, thus balked of its full utterance, is converted into a nasal inflection of the vowel—one which, perhaps, so increases toward the end of the vowel, or cleaves so especially to its concluding part, as to give the impression of a following element. I do not undertake now, any more than before, to decide the question dogmatically; but I am not at all satisfied by M. Bergaigne's theory, and think that he immensely overrates the value of his contribution to the discussion: he really does little more than record his individual vote in favor of one of the alternative solutions which I formerly proposed.

To M. Bergaigne's first brief article I sent a brief reply, which was published in a later number of the same *Mémoires* (ii. 194 ff.); but it was accompanied with extended comments by him, which seem to me to show the same excessive estimate of the strength of his position, and inability to understand and do justice to mine, which appeared in the original article. I have therefore thought it worth while to make this summary restatement of the case.

7. On Names for the Heart, Liver, and Lungs, in Various Languages, by Mr. J. Hammond Trumbull, of Hartford, Conn.

The Algonkin Indians designated their inferiors and servants by the epithet *Panis*, 'lung-y,' or 'all lungs.' Several tribes, not nearly connected, and speaking different languages, became known to Europeans and Anglo-Americans by this contemptuous appellation—now commonly written *Pawnees*. The figure of speech by which the name of the lungs marks depreciation or contempt is not exclusively Algonkin, or even American. We have in the Dakota *cha'ghu*, 'lungs,' *chaghu'ka*, 'a fool,' in the Arapaho, *ikun'a*, 'lungs, lights,' *kunani'ut*, 'cowardly,' in the Kechua of Peru, *surca*, 'lungs,' *surcanak*, 'a coward.' In the (African) Mpongwi, *ibobo* means both 'lung' and 'coward,' and, in the Lapp, we seem to find the same figure in *keppa*, 'pulmo,' and *keppes*, 'pauper.'

Similar agreement may be observed in the metaphoric uses to which the names of the heart and the liver have been put, in the speech of widely separated peoples. The origin of such metaphors must lie far back in language. They are, some of them, older than any known language. They do not, certainly, help us find our way to primeval unity of speech, but they are of interest by their suggestions of undiscovered laws of intellectual progress, under which speech has risen from objects of sense to conceptions of the supersensuous, from the material to the immaterial.

The association with the lungs of a notion of inferiority seems to have been established by observation of the contrast presented by the liver and lungs or 'lights' of animals. The former is heavy, compact, and dark colored; the latter, light, spongy, and pale. The liver was good for food, the lungs were of small value. The American Indians, who, at their feasts, distributed the portions with strict regard to the rank of guests, gave the lungs to the last and lowest. With the liver came to be associated ideas of strength, constancy, activity, courage; the lungs became types of weakness, levity, inactivity, cowardice. The former was made the seat of the passions and desires by which men come to mastery. The latter were mere servants of the body. In most European languages the lungs take their name from their lightness. The English 'lights' and 'lungs' are etymologically identical, and 'laggard' probably is nearly related to both.

The liver has very generally been regarded as the type of the passions and animal nature of man. The Orientals ascribed to it the principal agency in making the blood. Hence it became (Hebr.) 'the precious,' man's 'honor,' and 'glory.' This belief may be traced in many languages of the Old world. It is also found in the Polynesian, and in some American languages.

In the speech of almost all nations the heart has been recognized as the life-

center and source of vital energy. "Out of it are the issues of life." Its name has everywhere been transferred, 1. to whatever is central, or inmost; and, 2. to the moral nature and disposition. To the Semitic and Aryan philosophy, it was the seat of mental activity, as well as of physical energy; of all that belongs to the inner life, to that which perceives, thinks, wills, and desires.

Illustrations, from eastern and western languages, were given by the writer, showing how largely these names have contributed to the vocabulary. In the Chinese, for instance, of the 44,500 words in Kang-hi's Imperial Dictionary, 1097 begin with (or are formed on) the radical *sin*, 'heart.'

8. On the Exegesis and Criticism of the Old Testament, by Prof. Felix Adler, of Ithaca, N. Y.

Prof. Adler began by remarking that, apart from the high interest which it may claim on its own account, the study of the Hebrew Bible commands the attention of Orientalists because of its bearings on the history of those ancient nations with whom the Israelites came into contact. A review was then given of some of the principal theories which have obtained in Germany during this century concerning the composition and authenticity of the "Books of Moses." The names of prominent critics: of Vater, Astruc, De Wette, Ewald, Tuch, Hupfeld, Boemer, Graf, and others, were mentioned, and their individual services briefly referred to. It was shown how the methods of studying the Old Testament, at first somewhat loose and defective, gradually gained in consistency and accuracy. The different accounts of the creation, certain difficulties in the history of Joseph, and other conflicting statements, led to the assumption that the Book of Genesis is a collection of fragments bound together by a single hand. A more plausible hypothesis represented Moses in the light of a compiler, who had before him a number of ancient documents, from one or the other of which he selected for the purpose of his narrative, as the occasion seemed to warrant. Again, it was explained that the prevalence of different names of the Deity (Elohim and Jehovah), in many passages of Genesis, is not to be ascribed to a difference in the authorship of these passages, but is rather due to a distinct meaning attaching to each of the divine names. The same author might naturally change his terminology to suit the subject in hand. Accepting the fact that several authors had borne a part in the composition of the Pentateuch, Tuch distinguished a single principal record, to which later additions had been added as supplements. The main distinctions arrived at by Hupfeld have come to be widely recognized. He divides the writers of the "Torah" into a first and a second Elohist and a Jehovist. An editor is required to combine these records. The Book of Deuteronomy has its separate author, which would make five; and a sixth is added, to whom is ascribed the work of joining Deuteronomy and a great part of Joshua to the Tetrateuch. Boemer endeavored to explain contradictory enactments and narrations, by referring them for their origin respectively to the hostile kingdoms of Israel and Judah. He had thus called attention to an instrument of criticism which was largely and very successfully employed in the writings of Geiger. Graf sought to disprove that the laws of Leviticus are older than those of Deuteronomy. He labors to show that the former are inapplicable to any state of things such as had existed in Israel before the period of the Babylonian captivity, and therefore fixes their date after the time of the exile. The important results which the vast learning and critical acumen of Geiger have achieved must be passed by for the present. His work can be so little appreciated without an intimate knowledge of the later writings of the Jews, that a detailed discussion of it must be reserved for some future occasion. Those who take a special interest in the progressive development of biblical exegesis will find a brief history of the modern schools of criticism in the prefatory postscript which Merx has added to the new edition of Tuch's "Genesis."

As far as negative results are concerned, we may safely say that the exegesis of the Hebrew Bible rests on an assured basis. With respect to positive assertions, being so far removed from the past that its reconstruction is not wholly possible, we should learn to be cautious. On comparing the dates fixed by two eminent scholars for the composition of the second Psalm, we find a difference of not less than a thousand years. The remarks made by a certain modern critic concerning

the 118th Psalm also furnish a good instance of the absurdities to which an exaggerated attempt at accuracy may lead. This Psalm, he tells us, refers to the conquest of Idumea by Alexander Jannæus, who forced his new subjects to embrace Judaism, much against their inclination. The 10th verse he accordingly translates, "in the name of God *I will circumcise them!*" What we want now is, that the study of the Bible be directed less to literary and more to historical research. Neither the people nor their religion sprang full-grown into existence, but both passed through a long process of growth and development. It should be our endeavor to recover the traces of this process as far as we may. The text on which we rely is on the whole excellent, and we owe thanks to those who copied the manuscripts, that they were scrupulous enough to preserve even the errors that had crept in, instead of correcting them with such light as they had. The recognition and rectification of these errors requires that careful attention continue to be given to the text. Prof. Adler threw in here a few suggested emendations. In the 15th chapter of Exodus, the verses 11 and 12 have changed places, it being quite clear that "the earth swallowed them" corresponds to "the sea covered them" in verse 10. Psalm lxxi. 3 should be corrected according to Psalm xxxi. 4 (3d verse in Hebrew), and should read *beth mezudoth*; the mistake occurring from improperly dividing the line into words. In Job xxxiii. 21, a state of great emaciation is described. "His bones were dry, they were not seen," is hardly an adequate expression. A better rendering, taking *raah* for *ravah*, in which sense it is sometimes used, would be, "his bones were dry because they had no drink." A similar rendering of the passage has already been given by the distinguished Orientalist M. Dérenbourg.

Prof. Adler concluded with discussing certain points in the history of development of the Hebrew race and its institutions, especially the growth of monotheism out of an earlier idolatry; and he offered a conjectural explanation of the story of Achan, in Joshua.

This communication elicited remarks from more than one of the other members present, in criticism of and dissent from some of the author's views.

9. On the Identity of the Hebrew Shaddai with the Egyptian Sati or Set, by Prof. J. W. Jenks, of Newtonville, Mass.

10. On Muhammadan Art—a Translation from Dr. Carl Schnaase's *Geschichte der bildenden Künste*, with Remarks and Criticisms, by Prof. E. E. Salisbury, of New Haven.

The intention of this paper was, especially, to assist in penetrating into the genius of Islâm; and, although the German author must be charged with an oversubtlety in some of his generalizations, in explanation of art-forms, and seems sometimes not to have duly discriminated, for the purpose of a just estimate of the bearing and object of Muhammad's own teaching, between primitive Islâm and later developments of the system, yet there is much that is well-founded and instructive in his portraiture of the spiritual condition of Muhammadan nations, as such.

For example, he points out, as a fundamental fact, that rigid monotheism, or absolutism of Deity, the system of unconditional subjection of human personality, is not educating, but serves only as an exterior covering, "beneath which the wild power of the passions, and sensuality, runs its course all the more ruinously." Our author, however, justly recognizes that the principle of subjective freedom was asserted and operative in the earlier days of Islâm, while fatalism (a reaction, it may be suggested in passing, against that quest of second causes which drew the philosophers of Islâm away from orthodoxy) was a later development.

But the feeling for nature could not be wholly suppressed; and so, by the "psychological law that, where there is a one-sided predominance of abstraction, fancy is wilder, more turbulent, more violent," it manifested itself in "inclination for magical effect, for the wonderful and the unnatural," as well as for what is "elegant, light, and graceful"—qualified by, and contrasted with, a massive simplicity. In keeping with the doctrine of the divine unity, and of absolute predestination.

A form-creating power is wanting to Muhammadans; as appears in their poetry, in which "either mere abstraction of thought prevails, or an evanescent, self-

pleasing sensuousness;" and in their music, which lacks the indispensable conditions of musical perfection, in freedom from mortifying constraint, giving play to the imagination, together with a pleasure in beauty which does not take the form of spiritually deadening sensuous appetite.

Consequently, as respects architecture—the only properly formative art open to their efforts—if their edifices, "in their exterior, are at first imposing by their simplicity and bareness of ornament, we soon feel the emptiness of what is void of form, and seek for a broader carrying out and realization. And if we have found these last elements in decoration, and have surrendered ourselves, for some time, to the charm of an ingenious, story-like play of fancy [in architectural arabesque, alluring by its play of enigma, enchanting the soul by the vacillation of its lines, mocking it ever anew by intimation of hidden rules, giving it an unexpecting occupation, which can always be broken off, and always again renewed], there steals upon us quite the same feeling, still We move between the extremes of undeveloped groundwork and mere decoration: the important connection, by means of organized members, is wanting. While architecture should carry out rigid necessity into freedom, impart the form of what is organic and animated to that which simply meets a want, and is conformed to purpose—this office of the art is, from the very first, given up, and rigid necessity is joined to luxury, without intermediary. We find the sublime (although only in feeble accords) and the agreeable in richest development; the properly beautiful has no place."

It is to be regretted that so conscientious an attempt as is this work of Schnaase, not only to detail the historical growth, but to account for the origin, of the particular art-forms of all nations and times, in architecture, sculpture and painting, though, as has been said, sometimes over-refined in its analysis, should be so little known to English readers. A translation of it, with some abridgement, ought to find favor with the increasing class of art-students in the United States and England.

No further communications being offered, the Society, after passing a vote of thanks to the American Academy for the use of its rooms, adjourned to meet again in New York on the 28th of October next ensuing.